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GEOLOGY.—*Again on Pleistocene man at Vero, Florida.*[1] OLIVER P. HAY, Washington, D. C.

In a conversation held recently between an anthropological friend and myself about the finding of human remains in supposed Pleistocene deposits, about 11 years ago, at Vero, Florida,[2] he used an expression which implied that the investigations made there, the reports, and the subsequent discussions, proved disastrous for those who affirmed the presence there of Pleistocene man. This remark has prompted the writer to reconsider the case, after having devoted some years previously and the years since that time to the study of the Pleistocene vertebrates and of the Pleistocene geology of North America. I anticipate to say that I regard the investigations as far from having injured the case of Pleistocene man. In the symposium cited above there was no general agreement on the main question and it would be difficult to say who were farther apart in their conclusions, the geologists or the anthropologists.

When the geologists, the anthropologists and the paleontologists arrived on the spot they beheld a low-lying tract composed of thin beds of slightly consolidated materials which looked as if they might have accumulated within a few centuries and which offered for consideration a being almost universally looked upon as a "leitfossil" of the Recent epoch. The lowest stratum in view was a marine shell bed recognized by all as belonging to the Pleistocene, but by most of the company as appertaining to a late time in this epoch—late, because (1) this bed was composed almost wholly of mollusks apparently all of existing species and because (2) it reposed on a terrace,

[1] Received March 8, 1928.
[2] Journ. Geol. **25**: 1–62. 1917.

the youngest of at least three which, according to the prevailing theory, owed their existence to as many successive submergences during the Pleistocene beneath the sea. Overlying this marine marl was a freshwater deposit from 2 to 4 feet thick, composed of sand mingled with a little vegetable matter, some freshwater shells, many bones of land mammals, and a few of reptiles. This bed is known as No. 2. Lying upon this was found a stratum made up mostly of vegetable debris mingled with sand and containing various fossils. It formed a muck bed and was designated No. 3. It, as well as the underlying bed, No. 2, had evidently been deposited by the small stream which had, doubtless for many ages, wandered over the tract.

Now, under the conditions, material and psychological, how was it possible to find room in those thin deposits of sand and muck, for a Pleistocene creature whose skeleton and whose handiwork did not seem to differ from those of a red Indian?

Dr. Rollin T. Chamberlin, of the University of Chicago, made the main reports in opposition to the asserted presence of Pleistocene man.[3] He granted that the human bones found in strata Nos. 2 and 3 had been covered up as those deposits were laid down. "This formation [No. 2] contains human bones essentially *in situ* beyond reasonable doubt, together with the scattered bones of many extinct vertebrates."[4]

One can not be mistaken in saying that Chamberlin's efforts were expended in the endeavor to prove that the deposits containing evidences of man were of comparatively recent time. A feature which he regarded as of high importance was the discovery, in a bog immediately west of the fossiliferous locality, of a stratum from 2 to 4 feet thick, of a dark brown to black sandstone firmly indurated by oxides of iron and manganese. It was thought that the accumulation and induration of this may well have required considerable time. On examining the deposit where remains of man had been found (Sellards' No. 2 and No. 3) Chamberlin found numerous pebbles, "balls" and "cannon balls" of a similar dark sandstone. These he explained as fragments which had been brought down the creek and rolled on their passage into their globular form. He accordingly argued that the deposits holding the fossils and these balls were probably much more recent than the sandstone stratum of the bog. Also in his second report he retained his opinion that the sandstone had furnished the

[3] Journ. Geol. **25**: 25–39; 667–683. 1917.
[4] Journ. Geol. **25**: 27–28. 1917.

rough materials for the balls; hence "the oldest fill in the creek channel is notably younger than the bog deposit." However, one may argue on the other side. As is well known, sandstones saturated with water containing salts of iron and manganese, in the presence of organic materials may harden rapidly. On the west coast of Florida human skulls and skeletons have been found embedded in masses of bog iron, and the bones themselves are sometimes converted into limonite; and yet we are assured that these human remains are of comparatively recent age.[5] Nor is it necessary to suppose that irregular blocks of sandstone were rolled into balls as they were pushed down stream. Round concretionary masses are common occurrences in bog iron deposits and the formation of these may be effected rapidly. Released by erosion they would need no abrasion and would perhaps increase in size while rolling. It is still more probable, however, that the balls observed at Vero were engendered at the spot where they were discovered. At any rate, the bog sandstone and the creek beds may have been laid down in a relatively short, probably simultaneous time.

In his first report Dr. Chamberlin regarded the bog sandstone as also the source of most of the bones which were found in the creek beds. The animals had, he thought, lived, died, and left their skeletons in the sand before it had become consolidated. Later these bones had been eroded out and transported to their final resting place with the balls just described. This conception appeared to relegate the animals back in the Pleistocene to any convenient time and the deposition of the creek beds forward to any required late date. However, when on Dr. Chamberlin's second visit no bones could be found, either in the bog sandstone or in the creek on their way to the fossil-bearing beds, this hypothesis was abandoned. "The solution of the riddle of the mixture of bones of extinct animals with human bones and pottery was therefore sought on other lines." The critical problem was left "still crying for a satisfactory explanation."

In seeking a solution of the problem Dr. Chamberlin fell into various errors. He appeared obliged to assume a late date for the animals and for man. "Both of these deposits [No. 2 and No. 3] were late in the history of the formations of the region, and the oldest of these formations bears both a paleontological and a topographical aspect of recency."[6] In speaking of the marine coquina deposit he says that it does not bear evidence of great age, its shells being all of living species;

[5] Bur. Ethn. Bull. 33: 64–66.
[6] Journ. Geol. 25: 673. 1917.

and he cites the assignment, by geologists, of the terrace on which the coquina reposes to the late Pleistocene. It would have been instructive to tell us what Pleistocene deposits of mollusks are not composed of practically all existing species. The Upper Pliocene of England contains from 90 to 95 per cent of living species of mollusks and this Upper Pliocene corresponds to the lower portion of the American Pleistocene. Dr. Ralph Arnold[7] found in the Pleistocene Upper San Pedro beds, near Los Angeles, abundant molluscan species of which only 9.5 per cent were extinct. In Dr. W. C. Mansfield's list of mollusks[8] are recognized 61 species. Of these there are 19 species (exclusive of young and imperfect specimens) which are not indicated as occurring in the recent fauna. Certainly not all of these are extinct; but no one, I think, can affirm that none of them are. If 6 out of this lot are extinct the percentage will be 10; if only 3 are extinct the percentage will be 5. Another piece of evidence in favor of the early Pleistocene age of the Anastasia marl is the discovery in it of a bone of a camel, as reported by Sellards. What stands in the way of referring the Anacostia marl to the lower Pleistocene?

Dr. Chamberlin fell also into the error of accepting without further investigation the view that the terrace was a late Pleistocene marine formation. It may be permitted to call it the youngest terrace, but that does not fix its place in the epoch. Neither it nor the terraces above it are of marine origin. This is demonstrated by the total absence of marine fossils in all of them, except where local sinkings of the coast have occurred since the formation of the last terrace; and these depressions amount to only a few feet. Had those terraces been submerged they would have been filled with mollusks. Similar terraces are common in Europe along the coasts and many rivers, and on our western coast, and they abound in fossils.[9] Our east coast terraces are of river origin and were laid down in probably the earliest Pleistocene when the continent stood at a much higher elevation than now. It was probably at this time when the now submarine channel of Hudson River was excavated and the channels of many of our other great rivers were cut deep, to be refilled at a later time. Drs. T. C. Chamberlin and R. D. Salisbury[10] reject the marine theory of the terraces along our Atlantic coast. The reader ought to peruse, on

[7] Mem. Calif. Acad. Sci. 3. 1903.
[8] Fla. Geol. Surv., 9th Ann. Rept., p. 78.
[9] See HAUG, *Traité de géologie*, and ARNOLD, Mem. Calif. Acad. Sci. 3. 1903.
[10] *Text Book of Geology* 3: 452–454. 1906.

pages 412 to 414 of the 15th volume of the *Journal of Geology*, a review signed T. C. C.,[11] in order to obtain that writer's opinion about the marine origin of the terraces.

Dr. R. T. Chamberlin further assumed that the animal remains were swept by floods into the positions they occupied. No proof can be afforded that a single bone was thus carried into those creek deposits, although this transportation would not involve their belonging to a Pleistocene stage older than that of the deposit No. 2. However, the animals found there probably died not far distant.

In his efforts to prove the recency of the mammalian remains and the deposits at Vero, Dr. Chamberlin hit upon two ideas which have come to other minds since that time, if not before, and which appear to have given them much comfort. These are (1) that the southern climate was better adapted for mammalian life than that of the northern States and (2) that the mammalian fauna existed longer there than it did elsewhere. These notions appear to inspire a sort of poetical feeling, for the conditions are spoken of almost always as "that genial southern clime" and the animals are tenderly mentioned as "lingering longer there."

Doubtless during the Wisconsin glacial stage the mammals of the northern regions were forced southward, even into Florida and Texas. Reindeer reached Kentucky, musk-oxen migrated to Oklahoma, *Elephas boreus* (*E. primigenius*, of authors) probably strayed as far south as Florida and Texas, and so with many other northern species. When, however, the glacier retreated these animals did not remain there, but they kept as near the glacial front as they found it comfortable. Mastodons and certain elephants doubtless lived in Florida during the wane of the Wisconsin stage, but there is not a whit of evidence that they lived there at a later time than they did in New York or Michigan. For a reindeer and a musk-ox the genial climate is the one which furnishes plenty of snow and the kind of food they need.

Now as to the matter of lingering, it is a certainty that many of the mammals found in the Pleistocene beds at Vero, Peace Creek, Melbourne and many other places in Florida did linger there and elsewhere and become extinct only at a later time. *Mylodon*, one or more species of tapirs, the great ox *Bison latifrons*, and *Equus complicatus* appear to have lived on until the Sangamon interglacial. The American mastodon, *Elephas columbi*, and the giant beaver lived long after

[11] Journ. Geol. 15: 412–414. 1907.

the disappearance of the Wisconsin glacier and left their bones in the deposits overlying the drift. It is also true that many other species, specifically unchanged, are still lingering and they constitute the existing fauna of North America. With the species named above there existed at Vero, Peace Creek, and Melbourne *Megatherium, Chlamytherium, Glyptodon, Elephas imperator,* the Florida saber tooth tiger, and one or more camels. In regions farther west and northwest, as at Frederick, Oklahoma; Rock Creek, Texas; "Hay Springs" (Peters) Nebraska, and in the Aftonian deposits of western Iowa, there are found also numerous species of horses, camels, *Mastodon mirificus* and *Elephas imperator;* and these appear not to have existed anywhere after the first interglacial stage. Had they continued to exist their remains ought to be found in the deposits overlying the Kansan, the Illinoian, or the Wisconsin drifts. Outside of the drift region, in the Appalachian ranges from Lookout Mountain, Tennessee, to Frankstown, Blair County, Pennsylvania, in caves and fissures, have been collected numerous species of mammals of apparently mid-Pleistocene times, but none of those mentioned as being characteristic of the first interglacial stage. In northwestern Arkansas an abundant fauna has been discovered in a fissure, but among these were no *Elephas imperator,* no *Mastodon mirificus,* no camels, no *Glyptodon,* no *Megatherium,* no *Chlamytherium.* In the Mississippi embayment, extending from Cairo, Illinois, to the Gulf and on the south from western Louisiana to western Alabama, a very interesting fauna has been collected, consisting of mastodons, elephants, one or two species of horses, tapirs, megalonyx, mylodon, etc.; but again the forms which are taken to be peculiar to the first interglacial, or Aftonian, stage are not found. We are justified, I maintain, in believing that, instead of a few lingering here and there some hundreds of thousands of years, perhaps to conduct to the happy hunting grounds the spirit of some "mid-Recent" red man, they ceased existence near the close of the first interglacial, or at most did not live beyond the Kansan glacial. Therefore, I hold that the creek bed No. 2, at Vero, and its contents belong in the first, or Aftonian, stage of the Pleistocene.

During his second visit to Vero Dr. Chamberlin was especially engaged in determining, at the localities where human remains had been discovered, the relations of beds Nos. 2 and 3 at their plane of contact. His purpose was to learn whether the human remains were really found in No. 2 or in what he regarded as the very recent No. 3. As to the skeleton No. 1, the first one found, he thought that the 9

inches of brown sand overlying it was too thin to permit a safe conclusion.

At the locality of the second skeleton, where there had occurred more vigorous stream action, Dr. Chamberlin carried the plane of contact nearer the layer of shells. His conclusion was evidently that the human bones belonged in the muck layer or at least might have belonged there. That he proved this he certainly would not assert; nor would he perhaps regard it as necessary. The writer believes for the reasons stated above that it can not be successfully contested that the stratum No. 2 is of early Pleistocene age. In case the muck layer belongs to the Recent epoch we may inquire what was the condition of that little valley during the intervening 200,000 or 300,000 years? I think that no evidence can be furnished that additional deposits were laid down and afterwards removed. It is, as already mentioned, probable that the muck had been accumulating ever since the beginning of the Kansan glacial stage, and I believe that the fossils found testify to this proposition. If, now, this is true what becomes of deductions based by Dr. Chamberlin on the skillful work which he did at Vero?

Dr. Chamberlin[12] emphasizes the importance of the presence of the pottery found at Vero. No pottery was found in stratum No. 2. However, nobody has the knowledge or the authority to say that pottery was not used in America by Pleistocene man. As for myself, I would say that its presence in No. 3 is evidence that early man did use it. Recent revelations indicate that in America in Pleistocene times the art of working flint was far more advanced, in some tribes at least, than had been suspected. The same may be true as regards pottery.

It is the writer's conviction that Dr. Chamberlin erred as respects the age and origin of the coastal terraces, the age of the Anastasia marl, the ages of the creek beds and of the bog sandstone, the origin of the spherical concretions, the manner of accumulation of the bones, the composition and fate of the various elements of the fauna, and the position and age of the human remains. Nor can I give assent to any one of the four conditions set forth at the close of his second report.

The geologists appeared to be in agreement that there had passed between the deposition of stratum No. 2 and No. 3 no considerable lapse of time. In that case the apparent break may mark the begin-

[12] Journ. Geol. 25: 682. 1917.

ning of the Kansan glacial stage. The change of climate produced a more swampy condition of the little valley and made it a less agreeable resort for such of the larger animals as yet remained and there was a denser growth of plants. The muck accumulated slowly. There appears to be no evidence of either elevation or depression. If the time that has elapsed be taken as 300,000 years and the thickness as 50 inches the amount added would be one inch in six thousand years. The upper layers may be comparatively young; the lower, very old. While it is possible that some bones were washed up from the lower layer there is no necessity for granting it, for they belong to species which continued to live in that stage.

Dr. Aleš Hrdlička's theory of the presence of human bones in the deposits at Vero was short and simple. They were purposely buried there. No claim was made that there was any visible disturbance of the sand, marl, and muck such as would be caused by digging and refilling the grave. There might at first have been some unnatural mingling over the cadaver, but the materials would soon regain their former relations. He reported that evidences of this tendency to reestablish original conditions were observed already on the dump left by the steam excavator.

Dr. George Grant MacCurdy, of Yale University, recorded his conclusions in two papers.[13] In each article he figured three of the flint spalls collected by Dr. Sellards. Two were found in stratum No. 2. One of these was shaped somewhat like the blade of a broad ax. The height was one inch; the length of the thin edge was an inch and five-eighths. Dr. MacCurdy's explanation of its presence in the middle bed was that it had worked its way down by the aid of growing roots or burrowing animals. One may be curious to learn at what point of such a spall a root-cap could strike so as to guide it down through a bed of muck. More spalls were found in No. 2 than in the bed above it. Might not one as well assume that some had been washed up from the lower bed into the upper one? The number of animal burrows that have been dug in our broad land may be just a little short of infinite and arrow heads and spalls might work their way into these; but has any anthropologist ever found a flint weapon in such a situation? In the muck bed at Vero fragments of pottery were abundant. How has it happened that none of these were assisted to reach No. 2 either by roots or rodents? The specious value of such explanations was definitely exposed some years ago by the

[13] Journ. Geol. 25: 56–62. 1917; Am. Anthr. 19: 252–261. 1917.

investigations made at Trenton, New Jersey, by the American Museum of Natural History.

Dr. MacCurdy fell into the same error as Dr. R. T. Chamberlin and various other people, that of regarding the "fauna" found at Vero as an integral thing which existed for a while and later disappeared. I have shown already a number of genera which characterized the first interglacial stage as revealed at Vero and numerous localities and which appear at no later stage. Dr. MacCurdy mentions as occurring in the upper stratum (No. 3) at Vero *Elephas columbi*, *Mammut americanum*, *Chlamydotherium*, horse, and tapir. *Chlamydotherium* may have lived on for a while in the Kansan stage. It may have possessed some of the vitality of its near relative, the armadillo, which is still living in Texas. As for *Mammut americanum* and *Elephas columbi* and certain peccaries, they continued on probably all over the continent down close to or within the Recent. *Mylodon* and some species of tapirs and one of the horses found at Vero, *Equus complicatus*, and possibly *E. leidyi*, held on until after the Illinoian glacial stage.

After this article had been put in type the annoucement was made by Dr. J. W. Gidley, of the U. S. National Museum, that he had found, in two or three localities in Florida, human bones and artifacts definitely included within stratum No. 2. These discoveries ought to end the dispute about the relationship of man to this important deposit.

PALEONTOLOGY.—*Characters of the brachiopod genus* Lingulidiscina *Whitfield.*[1] GEORGE H. GIRTY, U. S. Geological Survey (Communicated by JOHN B. REESIDE, JR.).

Many years ago, in the course of studying certain faunas from northwestern Arkansas, it became necessary for me to deal with a large series of discinoid shells, and, while discussing the identification of the species I ventured to glance at the generic name that should be used for them. These shells belonged to the group for which Hall and Clarke had revived D'Orbigny's term *Orbiculoidea*, but it appeared to me that on their own showing *Orbiculoidea* was a synonym of *Schizotreta*. Under these circumstances I cast about for some name that was already in the literature rather than propose a new one, and provisionally adopted *Lingulidiscina* Whitfield. Now Whitfield's

[1] Published by permission of the Director of the U. S. Geological Survey. Received March 3, 1928.

description of *Lingulidiscina*, if taken literally, would make that name inapplicable to the orbiculoideas of Hall and Clarke, but I had reasons for believing that the description was in certain respects not accurate. I was led to believe that *Lingulidiscina* could be used to replace *Orbiculoidea*, among other things, by the fact that Schuchert in his bibliography of American fossil Brachiopoda had included under *Lingulidiscina* a shell that I knew to be a characteristic *Orbiculoidea*, and as I had usually found Schuchert well informed and accurate, I concluded, without inquiry, that he must be in possession of some esoteric knowledge regarding those genera. My confidence in this instance now seems ill-judged in view of the singular compilation that passed as the genus *Lingulidiscina*. Thus we have (1) *Orbiculoidea newberryi* in which both valves are in agreement with *Orbiculoidea* as generally understood; (2) *Oehlertella pleurites*, in which the upper valve is like *Orbiculoidea* but the lower valve entirely different, the pedicle aperture being a notch in the margin instead of an oblique tubular perforation; and (3) *Lingulidiscina exilis* itself, in which the lower valve is like *Orbiculoidea*, but the upper valve different.

Some years after my comments on *Orbiculoidea*, in 1912 to be exact, Professor Prosser[2] took a hand in the *Orbiculoidea* question and quoted a letter from Professor Schuchert to the effect that *O. newberryi* was included under *Lingulidiscina* by mistake. This admission was perhaps unfortunate because otherwise Schuchert might lay claim to almost superhuman penetration in an allocation that, on the face of things far astray, now appears to be very close to the truth. Prosser not only made this allegation against my use of the name *Lingulidiscina* but seemed to think that Hall and Clarke were entirely justified in their use of the name *Orbiculoidea*. Though I could not agree with Prosser on this point, and though one of my reasons for substituting *Lingulidiscina* was shown to be fallacious, I continued to use *Lingulidiscina* until very recently, partly because I felt disinclined to reopen the discussion and partly because *Lingulidiscina* still seemed available on fairly good grounds.

Now Professor Schuchert's inclusion of *Orbiculoidea newberryi* under *Lingulidiscina* was not my only reason for thinking that *Lingulidiscina* could properly be employed for these Devonian and Carboniferous shells. Indeed, I found great difficulty in understanding how, as was said to be the case in *Lingulidiscina*, a brachial valve that had essentially the shape and general plan of construction of

[2] C. S. Prosser. Bull. Ohio Geol. Surv. (4) 15: 203. 1912.

Lingula could be mated with a pedicle valve that had essentially the shape and general plan of *Orbiculoidea* (*Discina*) in view of the fact that these plans are so unlike that the two genera are actually assigned to different orders of brachiopods, *Lingula* to the Atremata and *Orbiculoidea* to the Neotremata. I felt that Whitfield's characterization could hardly be taken literally and that as his figures show the pedicle valve to have typical discinoid characters, the brachial valve was probably of the same type, though possibly having an apex uncommonly near the posterior margin. These considerations appear not to have occurred to either Prosser or Schuchert, and neither of them seemingly tried to ascertain what the characters of *Lingulidiscina* really were. The facts could be ascertained only through an examination of the type specimens, and these, through the unfailing courtesy of the American Museum of Natural History, I have been able to study. My observations in this field seem worth recording even though I now accept *Orbiculoidea* as a valid name in the sense adopted by Hall and Clarke, for they help to establish the relations of *Lingulidiscina* to other genera, relations which Whitfield's diagnosis left more or less doubtful. If his diagnosis were taken without qualifications, *Lingulidiscina* could hardly be of lower standing than the type of a new family. One might even go a little further and say that a brachiopod in which one valve had a terminal beak with shell accretions only at the front and sides while the other valve had a central beak with shell accretions equal all around, could not possibly occur in nature.

The generic description of *Lingulidiscina* reads thus:

> "An inarticulate brachiopodous shell, in which the upper valve is linguloid in character, having a marginal or an essentially terminal beak, the accretions by growth being along the lateral and basal margins; lower valve discinoid in character and having its growth lines nearly equal on all sides of the initial point and perforated on the cardinal side by a byssal slit or opening, as in *Discina*. Shell structure as in *Lingula* and *Discina*. Muscular scars yet unknown. Type, *Lingula exilis*, Hall."[3]

The type species of *Lingulidiscina* is commonly quoted as *Lingula exilis* Hall. This is possibly in error. Hall figured two specimens of *L. exilis*, one of which was subsequently figured by Whitfield in illustration of the genus *Lingulidiscina*. Figure 8 of Hall was described as "a specimen with the beak imperfect"; figure 9 as "a more convex individual which may belong to the species." The language here employed clearly implies that Hall was in doubt about the specific

[3] R. P. WHITFIELD. Bull. Am. Mus. Nat. Hist. 3: 122. 1890.

identity of these two specimens and that the one shown by figure 8 should be considered as the type of *Lingula exilis*. On the other hand, it is the doubtful specimen that was later figured by Whitfield as belonging to *Lingulidiscina*. If Hall's two specimens are really conspecific with each other, and if the doubtful one is in turn really conspecific with the specimens that furnished the generic characters of *Lingulidiscina*, then *Lingula exilis* is in fact the type species of that genus. The doubtful specimen is among those loaned me by the American Museum of Natural History and I feel confident that it is an *Orbiculoidea*. The other and typical specimen of *Lingula exilis*, I have not seen, but the growth lines in Hall's figure suggest that it is really what he believed it to be—a large *Lingula*. If such is the case, *L. exilis* obviously is not the type species of *Lingulidiscina*. I have not thought it necessary to borrow the type specimen of *Lingula exilis* in order to form an opinion upon this point, for my inquiry is addressed at present more particularly to ascertaining the characters of *Lingulidiscina*, and these depend upon Whitfield's specimens and not on Hall's.

To sum up my conclusions regarding *Lingulidiscina* before commenting on the type material in detail: Whitfield's specimens are poorly preserved, probably exfoliated, certainly somewhat crushed, and certainly more or less broken at the margin. Both valves are constructed essentially as in *Orbiculoidea* of Hall and Clarke, though the apex of the upper valve is more excentric than is common in that genus. Whitfield's description is misleading, if taken literally, in saying that the valve is "linguloid in character, having a marginal or an essentially terminal beak, the accretions by growth being along the lateral and basal margins." Let us consider the type specimens in detail, first those representing the brachial valve:

Figures 1, 2, and 3 in Whitfield's description of the genus represent the same specimen, figures 1 and 2 being different views of the brachial valve. These figures, which show a shell shaped like *Lingula* with a beak apparently terminal at the pointed posterior end, are accurate enough in so far as they represent the specimen as it now is, but they are highly misleading in so far as the specimen is decidedly imperfect. The growth lines run out half way up the sides of the brachial valve indicating that the shell was broken in the marginal parts and that the posterior outline was originally much less pointed or at least that the present outline is far from being the true outline. The point represented in Whitfield's figure as a terminal beak appears to be the true apex of the valve, so that the apex must originally have been situated some distance from the margin.

Where discinoids were buried with both valves in conjunction they often suffered a lateral displacement due to compression, one valve projecting on one side, the other on the other. When such specimens are broken from the rock the fracture is likely to occur where the valves are in contact, causing them to be defective along opposite sides. This condition is apparently exemplified by the specimen shown by figures 1, 2, and 3, the brachial valve having slipped backward with the result that the brachial valve projected beyond the line of contact at the posterior end and the pedicle valve projected a corresponding distance at the anterior end. The evidence for this interpretation is as follows: The brachial valve, even in its broken condition extends considerably beyond what appears to be the true posterior margin of the pedicle valve. In addition to the specimen illustrated, however, we have the slab from which it was detached. This slab retains the impression of the pedicle valve and also, projecting downwards from it almost at right angles, a considerable strip of shell, which appears to be the marginal part of the overlapping brachial valve. This strip of shell, which surrounds the pedicle valve from part way up the left side to part way across the posterior end, where it is broken off, may, it is true, be a section of the pedicle valve itself, folded over at a sharp angle, but from the apparently small amount and general direction of the compression suffered this explanation is not so likely. In any event, the brachial valve is undoubtedly imperfect around the posterior margin and the idea conveyed by Whitfield's figures is highly misleading. They represent the specimen as it is, without showing that it is fragmentary, and they seem to bear out the generic diagnosis in a way that is most deceptive. They show, it is true, a shell that at first recalls some of the broader, more spatulate lingulas, but in *Lingula* it is the pedicle valve that projects and is pointed, and the brachial valve that is short and more blunt at the posterior end; here the relation is precisely reversed, the brachial valve is long and pointed, the pedicle valve short and rounded at the posterior margin. This is, to be sure, not at variance with what the description says, though it is at variance with what the description seems to imply—an agreement of some vital sort with *Lingula*. However this may be, the relation between the valves as they now exist in the specimen are in all essentials as they are shown in Whitfield's figure 3, the pedicle valve rounded across the posterior end, the brachial valve pointed and projecting well beyond it. Such, one can say with almost perfect safety, could not possibly be the original condition of any brachiopod shell and the fact affords clear evidence, if

only evidence of an abstract character, of serious imperfections in the specimen. It is conceivable that in shells constructed on the discinoid plan the upper valve might be so oblique that its apical point projected beyond the pedicle valve, but the margins of the two valves would of necessity have the same outline.

The original of Whitfield's figures 5 and 6 is a brachial valve which has a depressed convex shape and a nearly circular outline. It is apparently unbroken around the posterior margin. This specimen is not unfairly represented by Whitfield's figure 6, but his figure 5, which is a side view, seems to be faulty. It represents the convexity as too low and the slope to the posterior margin as not sufficiently abrupt. The specimen itself gives indication of having been flattened, for it is dissected by numerous cracks. Whether on account of this, or of other imperfections, the exact location of the beak is hard to determine. It is perhaps more clearly seen if the valve is viewed from the side than if it is viewed from above, and is recognizable more by an abrupt change of direction in the outline from a gentle curve to a straight, steep descent backward than by a pointed prominence on the surface. Thus determined the apex appears to be a little posterior to the point of greatest convexity and to be situated some little distance up from the posterior margin (about 3 mm.). When the valve is viewed from above, however, the beak through foreshortening appears much more marginal.

Whitfield's figure 7 represents the original specimen fairly well, although it fails to allow sufficiently for the breakage that is clearly indicated at the right side, so that the true shape was more nearly circular than it is represented. The beak, as shown in the figure, is probably the true beak, but, as the legend states, the shell is folded inward at the anterior end so that the beak is not marginal as one might infer from the figure, but well up from the margin. Perhaps even more of the infolded shell was originally present than is actually uncovered. At all events in shape this specimen seems to have been very similar to the one last considered, for the convexity was low and the beak situated posterior to the highest point and not very far from the posterior margin, though it was by no means marginal.

Figure 8 of Whitfield represents the specimen that Hall doubtfully referred to *Lingula exilis*. The specimen is too imperfect to yield any facts relative to the genus *Lingulidiscina*, but it is without much doubt a discinoid shell instead of a *Lingula* and it may well belong to the same species as Whitfield's other types.

In addition to the specimens actually figured by Whitfield there was

sent to me as part of the typical material another brachial valve whose characters appear to have been essentially like those of the brachial valves already considered. The outline was circular, the convexity low, the beak strongly posterior but not marginal. Finally the borrowed material includes one of the type specimens of *Orbiculoidea doria* (that shown by figure 22 of Plate 2, Hall). The label is inscribed "*Lingulidiscina exilis*, a type of *Discina doria* Hall." Whether this expresses Whitfield's opinion or some other I do not know. In any event according to my conception of the species as derived from Whitfield's specimens, the brachial valve of *Lingulidiscina exilis* must have approximated very closely that of *L. doria*, though on a larger scale, and the specimen of *L. doria* loaned to me may well be a young specimen of *L. exilis*.

Only two pedicle valves are included among Whitfield's typical specimens, both of them figured. One of them is fairly well shown by his figure 3, though as the projecting part of the brachial valve is figured with it, the reader may fail to appreciate that the pedicle valve is broadly rounded at the posterior end and is very nearly symmetrical with a generally elliptical outline, a fact to which allusion has already been made. The major part of the valve is essentially flat; the margins are slightly upturned, producing a gentle concavity, but the parts of the shell adjacent to the pedicle scar are strongly introverted. The details of this scar are obscured. The second pedicle valve (shown by figure 4 as a mold of the exterior) corresponds closely in character with the first one, aside from being more circular in outline. It is gently concave over most of its extent owing to upturned margins, and has a large, deeply introverted pedicle scar. Whether the pedicle had an exit through an oblique tube as in *Orbiculoidea* or through an open fissure as in *Oehlertella* is not clearly shown by either specimen. For my own part I am strongly of the opinion that it issued through an inclosed tube, partly because no marginal slit has been observed, but more especially because an intense deflection of the surface at the locus of the pedicle, such as we have in these shells would more naturally accompany an oblique tube than an open slit. Incidentally, the specimen last described is associated with its fellow valve, which, however, is highly imperfect. Without showing any characters clearly it tends to corroborate the characters ascertained from the more perfect brachial valves already commented on.

The superficial characters of these shells are to some extent a matter of surmise. It is perhaps remarkable that all the brachial valves appear nearly smooth whereas the pedicle valves show the thin,

widely spaced, concentric lirae so characteristic of *Orbiculoidea*. All the specimens are more or less macerated or exfoliated, but there is no apparent reason why from this cause the one valve should not be defaced equally with the other. It is possible that the lower or pedicle valve retained these markings while the brachial valve which protected it, but which was itself liable to all the chances of abrading and corroding substances, might have lost them. On the other hand, I recall having seen orbiculoideas which showed a similar variation in sculpture. In both valves the central region was marked in the usual manner by spaced concentric lirae, but these gradually became obsolete toward the margins, deteriorating into feeble rounded striae of growth. The obsolescence of the sculpture was commonly more pronounced on the brachial valve than on the pedicle valve. If among these lingulidiscinas several that retain the upper and lower valves in conjunction have the one almost smooth and the other sharply striated, others, especially Hall's specimen of *Lingula exilis*, are not without suggestion of the characteristic *Orbiculoidea* sculpture in a defaced condition.

To sum up the conclusions that seem to follow from my observations on the typical specimens of *Lingulidiscina:* The outline is as a rule almost circular, though elongated specimens occur in which the length is decidedly greater than the width.

The brachial valve was depressed-convex, with the beak obscure but in position posterior to the highest point and not far from the margin in that part, though obviously by no means marginal. The configuration is in general like that of *Orbiculoidea;* it may be somewhat extreme in the lowness of the arch and in the excentric position of the apex, but it is not without parallel among specimens that seem properly to belong in that genus.

The pedicle valve also appears to be constructed like that of *Orbiculoidea*. My observations do not establish this, although none bears evidence to the contrary. The pedicle slit is here especially in mind; the configuration otherwise is not quite typical, for whereas in *Orbiculoidea* the pedicle valve is gently convex rising to a point in the central part (see Hall and Clarke's figure on p. 125), in these shells the pedicle valve is faintly concave, though it has a strong deflection in the posterior quadrant if the pedicle scar is taken into account. In configuration this type would thus appear to be somewhat intermediate between *Orbiculoidea* and *Roemerella*, though it is much more like *Orbiculoidea*. On the other hand, many recognized orbiculoideas have a pedicle valve that is almost flat, or even faintly concave,

whether by nature or by accident it is hard to tell. Few that have come under my observation are so strongly arched as Hall and Clarke's diagrammatic figure.

The sculpture is in a general way that which is common to most Paleozoic discinoids. That of the pedicle valve appears to be perfectly normal but that of the brachial valve, if not defaced, has more or less degenerated from widely spaced sharply elevated concentric lines into relatively inconspicuous fascicles of growth lines.

From the facts as they appear to me I would not hesitate to describe Whitfield's shells as representing a somewhat unusual species of *Orbiculoidea*.

ZOOLOGY.—*A new species of the nemic genus* Syringolaimus; *with a note on the fossorium of nemas.*[1] N. A. COBB, U. S. Department of Agriculture.

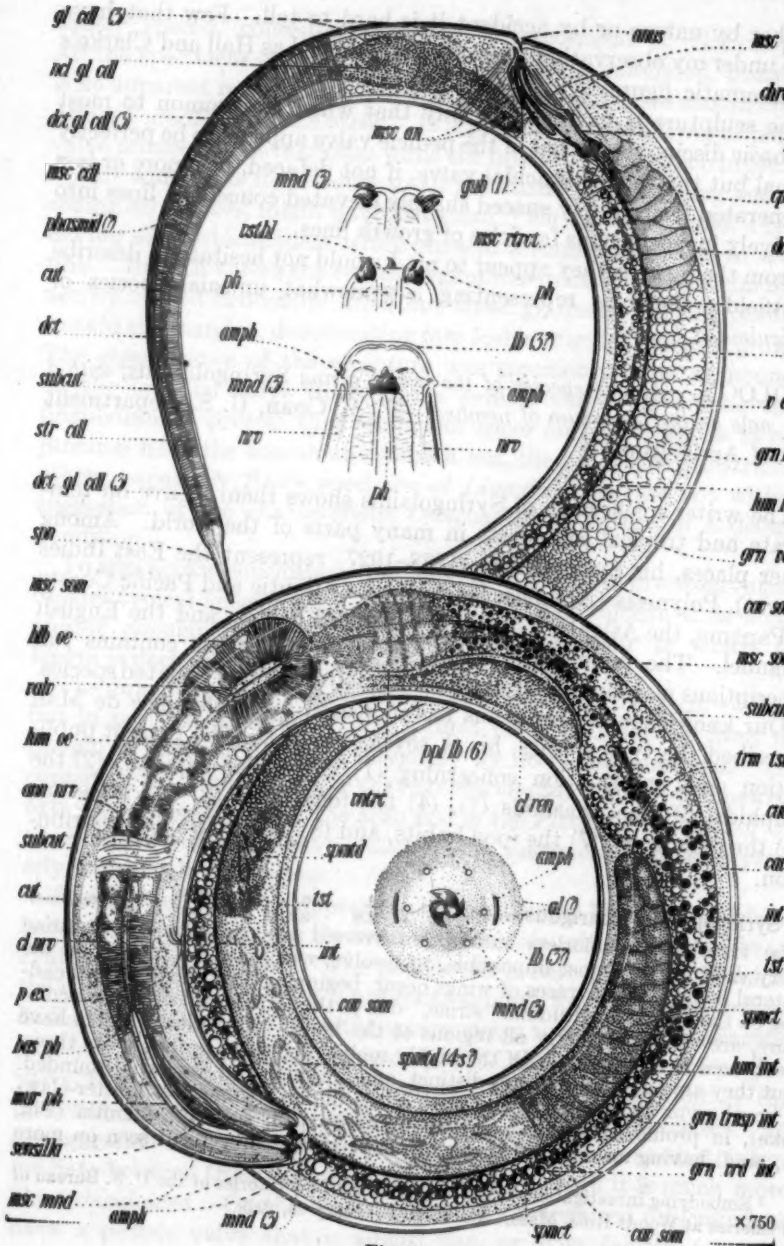
The writer's collection of Syringolaims shows them to live on temperate and tropical sea coasts in many parts of the world. Among other places, his Syringolaims (1888–1927) represent the East Indies (Larat), Polynesia (Noumea, Hawaii), the Atlantic and Pacific Coasts of Panama, the Atlantic Coast of the United States, and the English Channel. The manuscript record of these collections contains full descriptions of a number of new but unpublished closely related species.

Our knowledge of this genus has increased but little since de Man described the type species, his *S. striatocaudatus*. The present publication adds information concerning (1) the labial papillae, (2) the amphids, (3) the phasmids (?), (4) the fossorium, (5) the intestine, (6) the male gone, (7) the food habits, and (8) the geographic distribution.

Syringolaimus smarigdus, n.sp. $\frac{6.1}{2.7}$ ..... $\frac{12.}{2.9}$ ..... $\frac{20.}{3.2}$ ..... $\frac{13^{\circ}30'}{3.9}$ ..... $\frac{82.}{2.6}$ >0.76mm

The transparent, colorless cuticle is traversed by plain transverse striae very difficult, or almost impossible, to resolve, which are not altered on the lateral fields. Faint traces of wings occur, beginning near the head and ending on the tail. Longitudinal "striae," due to the attachment of the musculature, are visible in nearly all regions of the body. No series of pores have been seen in the cuticle. Of the highly mobile lips there probably are three, but they are no more than sub-distinct, and are small and somewhat rounded. The pharynx is armed in front with three duplex (somewhat lobster-claw-like), in profile somewhat inverted-comma-shaped, subacute odontia (Fig. 1, *mnd*) having an outward throw of about 180°, a movement seen on more

[1] Embodying investigations made largely at the Laboratories of the U. S. Bureau of Fisheries at Woods Hole, Mass. Received February 25, 1928.


gl cell (3)
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hm oe
ann tsv
subcut
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p ex
bas ph
msr ph
sensilla
msc mnd
amph
mnd (3)
anus
msc prtsl
chrd bst
msc an
mnd (3)
gub (1)
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msc rtrct
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lb (3?)
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nrv
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sp (2)
cph sp
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hm int
grn tas
cav som
msc som
subcut
trm tst
cut
cav
int
tst
spuct
hm int
grn trasp int
grn trvl int
cav som
spuct
ppl lb (6)
intrc
d rcn
spntd
amph
tst
al (2)
int
lb (3?)
cav som
mnd (3)
spntd (4s?)
× 750

Fig. 1

than one occasion to occur every second or so when the live nema was placed under the microscope under slight pressure (Fig. 1). The cross-section of the pharynx is round-triquetrous, almost circular, with faint subordinate markings in the middle of each side, indicating on the whole a hexagonal structure. There are no eyespots. The base of the pharynx may be surrounded by a very faint ellipsoidal swelling. There is only a faint pharyngeal muscular swelling, though there are fairly well developed mandibular muscles, lying along the outside of the pharynx (Fig. 1, *msc mnd*). There is a rather distinct but small conoid cardia, one-third as wide as the base of the neck, or less. The ventriculus stains differently from the remainder of the intestine, showing a distinct function to be discharged here; in the living condition however the ventriculus appears somewhat "structureless" (*vntrc*). The granules in the cells of the intestine are of several distinct kinds: some of them are colorless (*grn trnsp int*), others are emerald-green (*grn vrd int*)—hence the specific name *smarigdus;* none are birefringent. The content of the intestine is usually reddish or greenish, and often is derived specifically from an alga belonging to the family Ralfsiae (Fig. 3), among which specimens of *Syringolaimus smarigdus* are often found. There is no prerectum. From the somewhat elevated lips of the anus, of which the anterior lip is the more elevated, the cutinized rectum extends inward and forward a distance about equal to

Fig. 2.—Snails, natural size, covered with a very dark green "pile" or "felt" consisting of microscopic algae. The nature of this growth is illustrated in Fig. 3.

two-thirds of the corresponding body diameter. The lateral chords enlarge from one-fifth (terminad) to one-half (mediad) as wide as the body. From the medium-sized continuous vulva, the cutinized vagina leads inward at right angles to the ventral surface three-fifths the way across the body. The uteri contain only one egg at a time, are straight, three to four times as long as the body is wide, and from one-fourth to one-sixth as wide as long. The two opposite, equal, symmetrically arranged ovaries, about half as wide as the body, are reflexed about two-thirds the distance back to the vulva and contain ten to fifteen ova, mostly in single file. The elongate egg may be 3 to 4 body-widths long, appears relatively narrow, and seems to be deposited before segmentation.

6.5 11. 19. 47–51 84. 0.85mm
2.8 3.1 3.4 3.3 2.2

The single gubernaculum (*gub*) may consist of two arcuate, subslender, parallel, amalgamated pieces, and is rather closely applied to the spicula. Phasmids(?) (Fig. 1) occur on the

Fig. 1.—Male of *Syringolaimus smarigdus*, together with four different views of the head end. Below, a diagrammatic drawing of the front view of the head. Above, three sketches showing different attitudes of the "mandibles" or fossores. The fossores are also shown in both the other illustrations. The lettering of the illustrations consists of self-explanatory abbreviations arranged in the Latin order; *grn vrd int—granulum viride intestinalis,*—green granule of the intestine; etc.

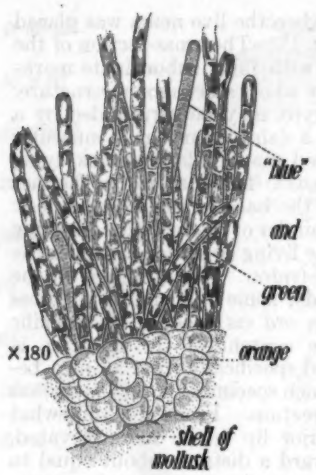


Fig. 3.—Slight portion of the algal growth from the snails shown in Fig. 2, broken or dissected away. Below is the shell of the mollusk. On it an "incrustation," orange in color, consisting of an alga belonging to the family Ralfsiae, probably to the genus *Ralfsia*. On this incrustation there is a thick felt-like growth, consisting of blue-green and yellow-green algae. *Syringolaimus smarigdus* feeds upon the orange-colored alga, which it can reach only by digging through the green algal growth above.

lateral lines near the middle of the tail on both sexes. While there is no distinct bursa, the cuticle is faintly thickened in the submedian region near the anus, possibly a very rudimentary bursa.

Habitat: Common among minute filamentous algae on the surface of marine mollusks, especially the snail *Alectrion obsoleta* (Fig. 2). Also found in sand on beaches, and in sand in several feet of water off shore. Woods Hole region, 1916 to 1927. There is good reason to suppose it ranges both north and south from Woods Hole along the Atlantic Coast. It occurs in beach sand from near Falmouth; and in clear white sand in five feet of water in a cove at the entrance to Buzzard's Bay; also at Waquoit, Mass., among algae on the shell of the living snail, *Alectrion obsoleta* (Nassa); and on the shells of live snails from the Eel Pond at Woods Hole. Its food seems always to be vegetable matter, and in many cases consists entirely of the contents of the cells of a microscopic alga belonging to the genus *Ralfsia* (?).

Outward Acting Nemic "Mandibles"

The writer's study of the attitudes in which the mouth parts became fixed led to the conclusion that in *Axonolaimus* and its relatives, as well as in a large number of other nemas, the onchia (and odontia) had an *outward throw*. If so, it was an obvious deduction that these organs were digging organs, for which the word fossor[2] seems indicated. This deduction led the writer long ago to introduce into generic names of such nemas root words indicating a digging function on the part of the mouth organs, as for instance in the genus names *Scaptrella*, *Diploscapter*.

It is, however, difficult to observe these organs in operation, and hence of interest to record that such organs have been seen in action in a Syringolaim (Fig. 1), and furthermore that *S. smarigdus* has been observed under conditions constituting strong additional circumstantial evidence that these organs are verily digging organs. *S.*

[2] *Fossor* (plural, *fossores;* collective, *fossorium*); a tool or organ used for digging, usually existing in a plurality and acting symmetrically outward from a plane or axis. Related to "fossorial"—said of animals that dig.

smarigdus is found in algal "incrustations" of the family Ralfsiae, and probably genus *Ralfsia*.[3] It is very apparent that the nema feeds upon the *Ralfsia*, for the color and structure of the contents of the cells of this alga are strikingly characteristic, and the intestinal content of the associated Syringolaims not only has exactly the same color, but frequently is otherwise of such a character that it could be derived *only* from the interior of the *Ralfsia* cells. Often, however, there are scattered foreign birefringent particles (carbonate of lime) mixed with the ingested food; but these birefringent particles are similar to those found *among* the filaments of the *Ralfsia*, and, taking into account the relative size of the mouth parts of the Syringolaim, it is very natural to suppose that some of this foreign matter would be taken in with the food.

No one had previously explained the precise nature of the mouth organs of *Syringolaimus*. They consist of three small, arcuate, more or less acute odontia with a *spirally outward throw*, well adapted to boring and digging (Fig. 1). Now, it so happens that the location and structure of the incrustation formed by the *Ralfsia* would require digging on the part of the nema in order to obtain food from it, for the *Ralfsia* incrustation on the snail shells (*Alectrion*) is usually overgrown with a thick comparatively impenetrable felt of filamentous green algae (Fig. 3); hence the *Ralfsia* can be reached by the *Syringolaimus only by digging*.

The snail, *Alectrion obsoleta* (Nassa), lives between tide-marks and hence twice daily is exposed to the air, and on each such occasion any algal growth on it naturally dries up more or less. Here then is an additional complication in the environment of the Syringolaim—a highly and rapidly variable temperature and salinity. It is reasonable to suppose these unusual circumstances might give rise to a peculiar nemic form adapted to the environment. Thus a clue is found to the marked peculiarities of form and structure noted in *Syringolaimus*.

[3] *Ralfsia; fide* Dr. I. F. Lewis.

ARCHEOLOGY.—*Check-stamped pottery from Alaska.*[1] HENRY B. COLLINS, JR., U. S. National Museum (Communicated by D. I. BUSHNELL, JR.)

Check-stamped pottery as an archeological type in America is restricted to the south Atlantic and Gulf region from North Carolina to western Louisiana, and the only modern Indians known to have made it were the Cherokee. It was with some surprise, therefore, that while cataloging the material collected by Dr. Aleš Hrdlička in Alaska in 1926, I found a sherd of otherwise typically Eskimo pottery bearing a check-stamp design. This sherd was picked up on the beach east of Cape Nome on Norton Sound.

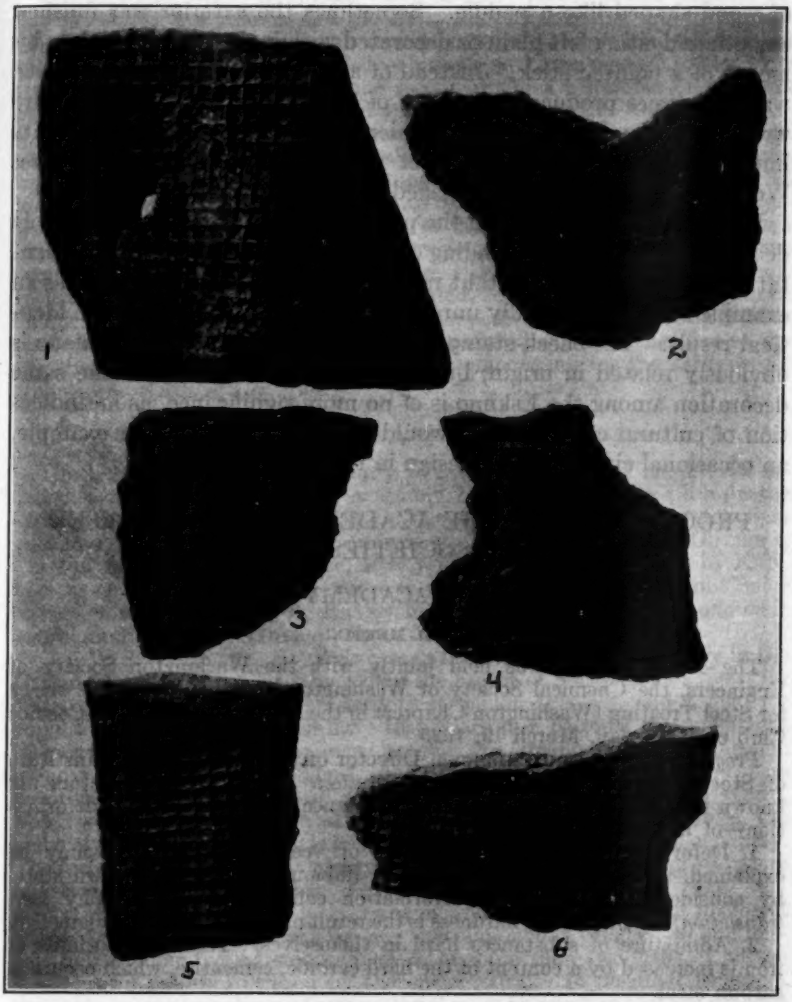
Last summer, while engaged in anthropological work on Nunivak Islands, Alaska, I found more of this pottery. Some three dozen sherds, all apparently fragments of the same vessel, a large flat-bottomed jar, were found together, about a foot below the surface near the ruins of some old houses at the present village of Koot, on the northeastern end of Nunivak Island. A sherd of similar type was obtained from an Eskimo who claimed to have found it while making the excavation for a new house at the same village.

In the accompanying figures are shown three of these sherds, together with the one found by Dr. Hrdlička on Norton Sound, and for comparison, two sherds from southwestern Louisiana. The practical identity of these pieces, as far as ornamentation is concerned, is apparent. The ornamentation was produced by applying to the soft clay a wooden paddle or stamp, on the surface of which was carved a checkered pattern.

Most of the pottery from the Eskimo area is plain except for simple punctate, incised, or corrugated ornamentation about the rim. Sometimes, however, the entire outer surface of the vessel has a rough pitted appearance brought about by the application of a paddle less carefully carved than those which made the impressions on the pottery here illustrated.

The manufacture of pottery is no longer practiced among the Eskimo but the process has been described by Gordon, as related to him by an old eskimo at Cape Nome who had observed it. "A quantity of clay, procured from certain localities on the tundra, was reduced to a smooth paste by mixing with walrus blood and kneading it with the hands. A quantity of sand from the beach was added, together with fine

[1] Received March 7, 1928.



Figs. 1–3, potsherds from Nunivak Island, Alaska. Fig. 4, from Norton Sound, Alaska. Fig. 5–6, from southwestern Louisiana.

feathers from the breast of the ptarmigan. From this material the vessel was built up by means of the hands with the aid of a flat piece of wood shaped like a paddle. Sometimes the exterior was finished smooth and either left plain or decorated with incised lines and dots by means of a pointed stick. Instead of a smooth finish a pitted surface was sometimes produced by means of a roughly carved paddle or by wrapping the unbaked vessel in a piece of grass matting which left its impression. The finished product was then baked in a wood fire. Women, and not men, were engaged in this industry."[2]

More than merely showing the presence of a certain type of ceramic decoration in Alaska, the finding of this pottery, identical in ornamentation with that from a distant region, is believed to be of interest as an example of how two totally unrelated cultures have produced an identical result. The check-stamped pottery of Carolina and Louisiana is obviously related in origin, but the sporadic occurrence of the same decoration among the Eskimo is of no more significance, as an indication of cultural contact, than would be the presence of, for example, an occasional circle and dot design in the Gulf region.

PROCEEDINGS OF THE ACADEMY AND AFFILIATED SOCIETIES

THE ACADEMY

198TH MEETING

The 198th meeting was held jointly with the Washington Society of Engineers, the Chemical Society of Washington and the American Society for Steel Treating (Washington Chapter) in the Assembly Hall of the Cosmos Club on Thursday, March 19, 1925.

Program: Dr. CARL BENEDICKS, Director of the Metallographic Institute of Stockholm, *The theory of high speed steel.* Three different factors are known to increase the hardness—the resistance against permanent deformation—of a given metal.

1. Deformation at low temperature, or "coldworking." This may be explained, without any assumption of a dubious "amorphous," hard state, by considering the mode of deformation called in crystallography *twin translation;* the increase in hardness is the result of intercrossing twin lamellae.

2. Admixture of substances hard in themselves. Thus the hardness of iron is increased by a content of the hard carbide, cementite, which occurs in carbon steel.

3. The occurrence of a substance in the state of solid solution. In 1901 the speaker pointed out that the solid solutions of the metals are much harder than the metals themselves; Kournakow has since shown this to be true for solid solutions in general. The speaker further, in his thesis of 1904, pointed

[2] G. B. GORDON. *Notes on the western Eskimo.* Trans. Dept. Arch. Univ. Pa. 11 (1): 83–84. 1906.

out that a metal is *hardened* if another element is made to occur in it as a supersaturated solid solution.

In every case where the metal—such as iron—possesses two different allotropic states, the gamma and the alpha states, quenching may cause two different solid solutions to occur simultaneously: a *supercooled* solution (as "austenite") and a *supersaturated* solution (as "martensite"). The slow transformation of the first into the second explains a number of hardness problems which otherwise demand special assumptions.

For high speed steel it has been established by X-ray analysis that tungsten possesses the same atomic lattice as alpha iron, differing from that of gamma iron. Hence it was natural that the tungsten is more easily mixed with alpha iron than with gamma iron. According to a general rule of physical chemistry, this implies that the stable region of the alpha iron was raised to much higher temperatures than would otherwise prevail. The alpha iron thus acquired a stability at high temperature that would not otherwise occur. In the case of carbon steel, a supersaturated, hard solid solution is obtained on quenching the high speed steel from a temperature so high that its carbide phase is dissolved. The main difference produced is the raising, due to the presence of the tungsten, of the range of temperature for the existence of alpha iron. (*Author's abstract.*)

199TH MEETING

The 199th meeting was held jointly with the Washington Chapter, American Society of Foresters, in the Assembly Hall of the Cosmos Club on the evening of Thursday, April 16, 1925. The general subject was a *Symposium on Forest Science.* The opening address was given by Col. W. B. GREELEY, Chief of the U. S. Forest Service, after which addresses were given by E. N. MUNNS on *Timber growing and protection from fire,* by H. S. BETTS on *Timber utilization and by-products,* by HAVEN METCALF on *Forest diseases and their control,* and by T. E. SNYDER on *Forest insect pests and their control.*

Col. W. B. GREELEY, *Forest Science in the United States.* Forestry in the United States thus far has largely had the aspect of a crusade, a subject for state or federal legislation, an activity of public agencies, and a field for popular education. These all have their place in a national movement but they represent only a preliminary stage. We are now entering a much broader phase of forestry. The United States has used up two-thirds of its virgin timber supply. Like all the progressive countries of the Old World, which went through an identical process of depleting natural resources, the value of wood in this country is creating a demand for a new source of supply. The force behind the current progress in forestry is becoming more and more largely one of cold economics.

It is perhaps worth while to take the measure of the job cut out for forest science in the United States. We now consume about twenty-two and one-half billion cubic feet of wood annually, or more than two-fifths of all the forest-grown material utilized by the entire world. In no great length of time this enormous volume of raw material must be produced by the practice of forestry. It is equivalent to about 280 million tons of material a year. This is nearly four times the tonnage of iron ore produced in the United States and well over ten times the annual production of cement. The quantity of wood required annually by the United States is over twice the total annual production of all cereal crops.

The size of the forestry problem of the United States is not, however,

gauged correctly simply in terms of raw material. Another very important side of it is the effective use of land. About one-fourth of the soil of the United States is now either forest land or land apparently fit only for producing wood. There is small prospect that this area will be materially reduced, at least for many years to come. The profitable employment of land and the rural prosperity that depends upon it loom just as large in our national economy as the problem of providing a sufficient supply of forest-grown materials.

The task of forest science in America is to create the basis of ascertained facts upon which and only upon which this far-reaching economic evolution can be soundly predicated. We often compare forestry with agriculture, and in sketching the field of forest science analogies may readily be drawn with the development of agricultural science. Forestry, like agriculture, involves (1) the protection of crops from destructive agencies, (2) crop culture and the betterment of yields in quantity and quality, (3) the utilization and marketing of crops, with a view particularly to the most effective conversion of raw materials into commodities of commerce, and (4) the economics of crop production and land use which guide sound investments of capital and labor.

The protective phase of forest science deals with the preservation of both old and growing timber from destruction or serious injury by fire, insect pests, and disease. It is not readily appreciated perhaps that there is an important scientific aspect to the protection of forest areas from fire.

Forest insects and tree diseases offer a large field for the expert entomologist and pathologist. Not only are the aggregate losses of forest-grown material enormous; not only must reasonable security from such losses be provided for the capital and labor invested in timber culture; in many instances the productive side of forest science cannot be soundly developed without direct correlation with the prevention or control of destructive pests. And on the other side it seems probable that much of the loss from forest insects or diseases will be effectively reduced only by proper methods of silviculture or timber management.

The second division of forest science, so to speak, is timber culture. In this as in the protective side of forestry there is an immense field for creative work in America. While certain principles and much in the way of practical experience can be borrowed advantageously from the Old World, the silviculture of the United States can only be an outgrowth of its own soil, climatic, and biological factors. Its problems include the more or less distinct growth requirements of at least 100 commercial species of trees.

American foresters have undertaken to develop and apply the art of timber culture in our woodlands from the more obvious facts deduced through superficial observation and the principles of forestry developed by European experience. It has thus far been more a reliance upon faith than upon exact knowledge of the sort which backs up the undertakings of the chemist or the engineer. We cannot yet predict with any close accuracy the yields of timber to be obtained in many portions of the United States under even the simpler and more rudimentary principles of timber culture.

The third phase of forest science deals with the utilization of timber after it has been grown. Wastefulness in the exploitation of American forests and in the manufacture of their products has been engendered by the very abundance and cheapness of our virgin timber. More economical use of wood is just as essential a part of forest conservation as the growing of new crops of timber.

This may appear to be a field for engineering or chemical or physical

science rather than forest science. Unquestionably it will call for the expert services of many trained engineers, chemists, and physicists. However you may view it as a field of scientific endeavor, its problems are closely linked with those of forestry.

And finally, forest science will not be complete without a comprehensive understanding of the economics of timber growing, timber transportation, and timber use. The business man wants to know whether forestry will pay, particularly since timber growing enterprises represent investments of capital over such extended periods. Only genuine scientific study can develop the economic background of forestry in the United States.

We are appreciating more and more clearly that no group of scientists lives unto itself; and that no field of scientific work can be marked out by sharp barriers cutting it off from other fields. This is true particularly of forest science. Its lines of work and interest reach out at many points into the fields of pathology, entomology, plant physiology, chemistry, and the engineering sciences. It is fruitless to attempt to say where one domain ends and the other beings. The development of forestry needs the active participation of many scientific groups and agencies, each marching under its own colors. (*Condensed from speaker's manuscript.*)

E. N. MUNNS, *Timber growing and protection from fire.* Timber growing research has been conducted for the most part at the federal forest experiment stations. The whole question of timber production is involved, including such things as nursery and planting practice, development of the best methods of harvesting the mature crop in order to insure the establishment of a new forest in the shortest possible time, and the determination of the yields which can be obtained at different ages and for different types of forest. Investigations have indicated that the Douglas fir regenerates after cutting from the seed which had been stored in the forest litter previous to cutting: that the western white pine seed can be held over in the forest soil up to six years: that it is necessary to save all the advance growth in the western yellow pine forests because climatic conditions are so severe. Investigations have shown that in various forest types the debris resulting from logging must be disposed of in different ways; in one region it can be scattered over the ground, in another it must be piled and burned, in another burned broadcast at certain seasons of the year. Already marked results have been obtained by following the practices indicated by the work of the forest experiment stations. One of the interesting recent developments has been in our knowledge of forest fires, how and why they burn, and how they may be better controlled. Detailed studies have indicated that forest fire weather can be predicted and that there is a close relationship between weather conditions and the spread of fire. As a result, there is now developing a technique of fire protection which is displacing the manual-labor method by one of scientific accuracy, and we are now better able to appraise the damage that fires do. The problem of the forest experiment station is that of how to obtain the maximum forest crop in the shortest possible time. (*Author's abstract.*)

WALTER D. LAMBERT, *Recording Secretary.*

THE GEOLOGICAL SOCIETY

434TH MEETING

The 434th meeting was held at the Cosmos Club, January 11, 1928, President HEWETT presiding.

Program: F. E. MATTHES: *Evidence of three glaciations in the Yosemite region.* In a previous communcation the speaker has set forth the facts which in his judgement prove conclusively that the Yosemite region has been subjected to glaciation twice during the Pleistocene epoch. He now desires to present certain evidences that would seem to point to a third stage of glaciation that antedated the others. These evidences were noted by Frank C. Calkins and himself as early as 1913, but they then seemed hardly conclusive enough to be made the basis of a positive statement. A recent reinspection of certain critical areas, however, leads the speaker to believe that there is warrant for at least a tentative recognition of this third, very early stage.

The evidence found thus far for three stages of glaciation in the Yosemite region may be summed up as follows:

The latest stage, which corresponds in all probability to the Wisconsin stage of the continental ice sheet, is indicated by a series of well preserved, sharp-crested moraines containing a large proportion of unweathered boulders. The rock surfaces that were overridden by the glaciers of this stage have suffered but little from weathering and still retain their polish and striae over considerable areas.

An earlier stage of glaciation that was marked by a much greater extension of the ice streams than was the Wisconsin stage is indicated by another series of moraines of greater volume but less well preserved than those mentioned. They are as a rule inconspicuous and difficult to trace because of the destruction of their crests and the smoothing of their flattened bodies by a surficial layer of sand derived from disintegrated boulders. Most of the boulders within these older moraines are enveloped by a ferruginous coating, and are decomposed and rust stained to a depth of at least half an inch. The rock surfaces that were planed by the glaciers of this earlier stage, far from retaining their polish, have been stripped of disintegrated rock to a depth of several feet. This is shown by the height of residual pedestals preserved under protecting boulders and still better by the height of dikes of resistant aplite that now project above the granite which forms the country rock. Some of the dikes on Moraine Dome stand from 7 to 10 feet high above the granite.

The third and earliest glaciation is believed to be indicated by isolated erratics situated at levels from 100 to 200 feet above the highest of the older moraines, and composed only of the most durable rock types such as quartzite and highly siliceous granite. In some places, as notably on the upland to the east of Mount Starr King, such isolated erratics occur on nearly level tracts where the washing action of rain water can not have been especially vigorous and where in general moraines would have a good chance to be preserved for a long time. They lie, moreover, in places where there is good reason to believe that fairly heavy moraines were once laid down. It is entirely probable, therefore, that these erratics are the sole remnants of moraines of much greater antiquity than those which make up the so-called older series. (*Author's abstract.*)

WILLIAM W. RUBEY: *Possible varves in marine Cretaceous shale in Wyoming.* Microscopic examination reveals laminations about 1/100 of an inch thick

in marine shale in many Upper Cretaceous formations in the Black Hills region. These laminations are of different kinds, marked by three types of alternations: (1) coarse and fine particles, (2) much and little organic matter, and (3) $CaCO_3$ and silt. Examples intermediate between (1) and (2) form a gradational series, with those laminations made by alternations of particle size the thicker, and those made by alternations of organic content the thinner. This gradation indicates that at least these two kinds of laminations were formed by the same process and during approximately equal time intervals.

Storms or floods might have caused the alternations of coarse and fine particles but the laminations marked by varying content of organic matter and lime seem to call for recurrent cycles of organic growth or temperature changes. The fact that thin laminations have been preserved indicates that sporadic storms rarely disturbed the sea floor and the regularity of the alternations suggests that the cause, whether storms or not, recurred periodically. Seasonal changes in temperature, rainfall, and food-supply or periodic shifts of marine currents probably afford the simplest explanation of all three kinds of alternations; and, of these two possibilities, seasonal changes appear the more likely.

It is conceivable that annual layers might have formed in the Upper Cretaceous rocks of the Black Hills region, for fossil wood and other evidence indicate that the climate was seasonal; and flocculation, which might possibly prevent the separation of sand and clay into coarse and fine layers, probably would not prevent the formation of layers rich in organic matter and lime. And once formed, thin layers might have been preserved, for the deepest part of the Upper Cretaceous interior sea probably lay near the present Black Hills; also, wave action may not have extended as deep during the widespread equable climates of Upper Cretaceous time as it does today.

The hypothesis that the laminations are annual is tested roughly by comparing the thickness of the observed laminations with the thickness that annual layers might be expected to have. Varves in glacial deposits from other regions are commonly much thicker but many varves in lake deposits and in some marine rocks are of about the same thickness as these laminations. Estimates of the expected thickness of varves in the Upper Cretaceous rocks near the Black Hills also appear to support the hypothesis. Three methods of estimation—(1) The probable area of land draining into the Upper Cretaceous sea and the supposed rate of erosion on the land, (2) the total thickness of Upper Cretaceous rocks in the region divided by Barrell's estimate of the number of years in the Upper Cretaceous, and (3) the rhythmic alternations in Upper Cretaceous rocks in eastern Colorado which Gilbert suggested were formed during precession cycles—all indicate annual layers only slightly thinner than the observed laminations. A modification of the second method (total thickness of Upper Cretaceous rocks divided by old estimates of Upper Cretaceous time based on the amount of salt in the ocean) indicates annual layers somewhat thicker than the observed laminations.

Thus, the laminations are of about the right thickness to be annual; they were not caused by daily variations or by cycles several thousand years long. However, this is no proof that they are annual for they might have formed every few months or years. In fact, more detailed comparisons (*a*) of the length of Upper Cretaceous time indicated by the laminations with that estimated by Barrell and (*b*) of the rate of eastward transgression of the Upper Cretaceous sea (calculated from the number of laminations and the distance of the transgression) with present rates of strand-like movements, suggest this latter possibility. Yet these two relatively small discrepancies

might be explained equally well (1) if the average thickness of the observed laminations is not typical of the series in the region, (2) if Barrell's estimate of Upper Cretaceous time is too long, or (3) if there are many inconspicuous unconformities or diastems in the stratigraphic section.

Whether or not the laminations are annual, they suggest some of the conditions of deposition. The degree of preservation of the laminations indicates that the fine grained, organic shales accumulated in deep water or at times of gentle winds. And if the different kinds of laminations formed during equal time intervals, their relative thicknesses indicate that, in general, the sandier shales accumulated more rapidly and the finer grained and more organic shales, more slowly. (*Author's abstract.*)

W. C. ALDEN: *The Gros Ventre (Wyoming) flood of 1927.*

435TH MEETING

The 435th meeting was held at the Cosmos Club, January 25, 1928, President HEWETT presiding.

Informal communications: A. J. COLLIER showed pictures, taken in Montana in 1927, of hailstones one and three-quarters inches in diameter and of the impressions they made on the ground by their impact. He inferred that some so-called fossil raindrop imprints were probably fossil hailstone imprints.

DAVID WHITE discussed the diagrammatic representation of the length of geologic periods and showed a diagram with the geologic time estimates of Holmes and Lawson plotted on a logarithmic spiral. (Diagram and note on pp. 201–203, vol. 18, this JOURNAL).

Program: Dr. E. C. ANDREWS, Government Geologist, New South Wales, Australia: *Geology of the Broken Hill district, Australia.*

436TH MEETING

The 436th meeting was held at the Cosmos Club, February 8, 1928, President HEWETT presiding.

Informal communcations: F. L. HESS described the occurrence near Hebron, Maine, of pollucite, a hydrous caesium aluminum metasilicate ($H_2Cs_4Al_4(SiO_3)_9$), associated with albite in pegmatites that contain lithium minerals. The pollucite is one of the last minerals to form in the pegmatites but it is partially replaced by albite. Blocks of pollucite that weigh several hundred pounds have been found at this locality. This abundance and the great resemblance to quartz led Mr. Hess to infer that pollucite may have been mistaken for quartz in pegmatites in other areas. The mineral has been a source of caesium for filaments in radio tubes.

Program: C. WYTHE COOKE and L. W. STEPHENSON: *The Eocene age of the supposed late Upper Cretaceous greensand marls of New Jersey.* The Hornerstown marl, the Vincentown sand, and the Manasquan marl, three formations of the Coast Plain of New Jersey that have heretofore been referred to the Upper Cretaceous series, are now correlated with the Eocene on the basis of a new analysis of their contained fauna and because of the transgressive overlap of the Hornerstown across formations of undoubted Upper Cretaceous age. Together with the overlying Shark River marl, the Eocene age of which has not been questioned, they appear to be approximately equivalent to the Pamunkey group (Eocene) of Maryland, which they resemble both in lithology and fauna. (*Author's abstract.*)

JAMES GILLULY: *Isostasy as a factor in Basin Range faulting in the Oquirrh Range, Utah.* The statement has frequently been made that given an original uplift of a mountain mass above the adjacent country, the resulting transfer of material by erosion from the uplifted mass and deposition upon the adjoining territory will tend to continue the differential movement of the two earth blocks in accordance with the isostatic principle. There is no question that such is the tendency. Quantitative measure of the actual effect of the transfer in continuing the relative movement is, however, rarely available. It is the purpose of the present paper to present some results of a study of this question with respect to the Oquirrh Range, Utah.

The Oquirrh Range is a typical Basin Range, bounded along its western base by normal faults which are demonstrable on structural and stratigraphic as well as physiographic grounds. The recognition of a submature pre-faulting topography permits the reconstruction of the pre-faulting surface, within fairly satisfactory limits. The reconstruction of this pre-faulting topography over the part of the range which is tributary to Rush Valley permits the approximate determination of the volume of rock eroded from the mountain block since the uplift began. This computation reveals the erosion from this part of the range of a rock layer with an average depth of 1100 feet. Assuming that all this material was deposited within part of Rush Valley which now contains fans sloping out from the Oquirrh Range (which assumption is contrary to the exact facts, but tends to favor the efficacy of isostasy) we find that the valley block has sustained loading equivalent to 670 feet of solid rock. The couple set up between the mountain and valley block could not, then, account for more than 1770 feet of the difference in elevation between them, even if we assume that the subcrustal material (which is on the assumption of perfect compensation transferred at depth in the reverse direction) has a density equal to that of the surface rock. If the subcrustal material has a density of 3.3, which is a more probable value, the couple set up by erosion can only account for about 1,550 feet of the observed 3,000 to 5,000 feet difference of elevation of mountain and valley block. Obviously, we must concede that surface transfer of material is of little moment in maintaining the elevation of the Oquirrh Range and that whatever caused the original uplift relative to the valley blocks is still the effective agent in maintaining their relative positions.

Post-Bonneville faulting along the west front of the Oquirrh Range probably averages about 20 feet in displacement. If we estimate 25,000 years as this duration of post-Bonneville time and take Dole and Stabler's figures for the rate of erosion in the Great Basin province (using a factor of 100 per cent to take account of the traction load, neglected by Dole and Stabler) we arrive at a maximum estimate of post-Bonneville erosion of 3 feet. Study of the physiography of the fans reveals that this figure is absurdly high, but even if we accept it, we find it wholly inadequate as a cause of the fault movement which is fully six times as great.

Attention is called to the great work of Gilbert in connection with his study of Lake Bonneville, in which he attacked this problem from several other viewpoints, including particularly the negative evidence afforded by the waterload to which the valley blocks were subjected in Bonneville time. His conclusion that the Basin Ranges are sustained by the strength of the earth's crust seems to be supported by the study of the Oquirrh Range. In conclusion it is pointed out that the limit of load which Bowie concludes from geodetic evidence may be carried by the crust without compensation is large enough to permit this conclusion not only with regard to the Oquirrh Range,

but also to practically any other single Basin Range with the exception of the Sierra Nevada and possibly the Wasatch. There is no reason to question the isostatic balance of the province as a whole, but it seems likely that its individual ranges are not compensated by deficiency in density of the rock columns directly beneath them. (*Author's abstract.*)

T. S. LOVERING: *Geology of the Moffat Tunnel, Colorado.* The Moffat Tunnel cuts through the continental divide on D. & S. L. R. R. about 50 miles west of Denver, Colorado. It is a standard railroad tunnel 16 by 24 feet in cross section and over six miles in length. The original estimate of its cost, $5,250,000, was based on the supposition that nothing but hard rock would be encountered. Unfortunately several thousand feet had to be driven through material so soft that costs were increased fourfold.

The writer made a study of the geology of the tunnel in November, 1927. This study showed that a relatively weak schist had been brought against granite and injection gneiss by a profound fault about 2¼ miles east of the West Portal. Substantial support was needed throughout most of the tunnel driven in the schist but in the granite and gneiss on the east of the fault there are long stretches of unsupported rock tunnel. In the pilot tunnel much of the schist is supported by timbering which needs renewal about once a year. Anyone familiar with mining in the nearby schist areas would find nothing unexpected in the character and behavior of the rock encountered in this section of the tunnel.

However, some surprisingly bad ground was encountered near the large fault and soft swelling ground extends about 700 feet to the west and 300 feet to the east of the main break. In this zone the walls, roof, and floor steadily close in at a rate varying up to a maximum of three inches a day. Strongly reinforced concrete is necessary to withstand the pressures developed in this part of the tunnel. Little water was encountered but the rock is slightly moist when opened.

The rocks in this belt have been thoroughly shattered, and chalcedonic quartz and a clay-like mineral have developed as the cementing material of the minutely brecciated rock mass. Experiments on the elasticity of the rock indicate that relief from the rock pressure obtaining in the tunnel could not account for the swelling of the rock by elastic expansion. Experiments on the hydration of the rock indicate that absorption of water even by thoroughly dried samples of the bad ground causes practically no expansion. As the rock in the tunnel is already moist when first opened, swelling can not by caused by hydration. Tests show that although the rock is weak when dry it is even more devoid of strength when wet.

It is probable that the abundant flakes of moist clay in the rock act as a lubricant along the innumerable free surfaces in the brecciated mass. Thus the swelling of the ground probably expresses the integration of minute slippages between the extremely small fragments in the shattered granite and schist. In the fault zone the rock pressure is essentially all-sided or hydrostatic in its nature and causes a uniform inward movement of the bad ground from top, bottom, and sides of the tunnel. In the course of time the rate of swelling grows gradually less. Continued excavation and shaving of the tunnel walls probably relieve the strain in the nearby ground to a marked degree and these factors are chiefly responsible for the gradual improvement in conditions. The strengthening of the rock mass as it dries out is an additional factor working towards the same end. (*Author's abstract.*)

437TH MEETING

The 437th meeting was held at the Cosmos Club, February 22, 1928, President HEWETT presiding.

Informal communications: W. T. SCHALLER described briefly the organization of a sulphur company recently on trial in New Orleans for fraud.

Program: WILLIAM BOWIE: *Changes in geographic position in California as determined by triangulation.*

Discussed by Messrs. FERGUSON, BUTTS, and ALDEN.

C. S. ROSS: *Report on the studies of clays.* An investigation of the clay minerals, under the auspices of the National Research Council, is in progress in the United States Geological Survey laboratories with the coöperation of mineralogists of the National Museum and of Dr. Paul F. Kerr of Columbia University. This work thus far indicates that the important clay forming minerals are not large in number, and that a large proportion of those formerly described are not valid mineral species. The clays may be divided into 3 main groups. The kaolin group includes kaolinite, halloysite and the macroscopically crystalline form of halloysite. These have moderate indices of refraction, low birefringence, and good crystals are not rare.

The montmorillonite group contains montmorillonite, beidellite, nontronite, the iron bearing member of a beidellite-nontronite series; and two unnamed members of the group. The members of this group are hydrous aluminum silicates with variable proportion of MgO and CaO, and of Fe_2O_3 which latter replaces Al_2O_3. The silica ratios are as follows:

Montmorillonite	SiO_2,	Al_2O_3	= 5:1
Unnamed mineral	"	"	= 4:1
Beidellite	"	"	= 3:1
Unnamed mineral	"	"	= 2:1

The part played by MgO in the mineral composition is not fully known and must be further investigated.

A group of potash-bearing clays has been found but their study is in preliminary stages.

Dr. Kerr has studied the clays by means of X-ray diffraction patterns and thus supplemented optical and chemical studies and added important new information. It has been found that the clays, even though very fine grained, can be investigated by the research methods now at the disposal of the mineralogist. (*Author's abstract.*)

Discussed by Messrs. STOSE, MERWIN, RUBEY, and HEWETT.

H. S. WASHINGTON: *Review of Lacroix's paper on the Rocks of the Pacific Islands.* A review of "La Constitution Lithologique des Iles volcaniques de la Polynésie Australe," by A. Lacroix. This is a very important paper, embodying our present knowledge of the petrology and petrography of the lavas of the volcanic islands of the Pacific Ocean. Details cannot be gone into here, but it may be said that the main conclusions of Lacroix—who gives some 75 new and good chemical analyses of the lavas (many of islands hitherto unknown)—are in general agreement with the views of those who have made a study of the Pacific Island volcanoes. The most important conclusion, one that was known before from study of some of the island groups, is that practically all of them, while they are predominantly basaltic and andestic, show trachytic and other alkalic rocks. This connects the rocks of the Pacific volcanoes with those of the Atlantic and the Indian Oceans, and is adverse to the distinction between the "Pacific" and "Atlantic" clans of rocks. (*Author's abstract.*)

Discussed by Mr. BOWIE.

438TH MEETING

The 438th meeting was held at the Cosmos Club, March 14, 1928, President HEWETT presiding.

Informal communication: L. W. STEPHENSON displayed specimens of Upper Cretaceous chalky limestone of about Austin age from Guatemala, in which were closely associated the fossil remains of at least two species of marine shells of the genus *Inoceramus* and two species of very well preserved land plants, one a fertile pinnule of a fern and the other a twig of a coniferous tree.

Program: N. W. BASS: *The origin of the asymmetrical stream valleys of Kansas.* In Kansas, streams that flow eastwardly and those that flow westwardly are eroding their south banks; the north-facing slopes are steeper than the opposite slopes, and the profile of the interstream areas is asymmetrical, the north side being the shorter. Southward trending valleys do not show this asymmetry.

The principle of Ferrel's law that all streams in the northern hemisphere should exert pressure on their right banks does not apply because it is only eastward flowing streams that are eroding their right banks; westward flowing streams are crowding to their left banks and southward flowing streams appear to maintain a neutral course. Regional tilting southward could have caused the condition noted but the scant evidence indicates that it has not.

Certain climatic factors—more direct rays of the sun on southward-facing slopes; prevailing southerly winds, acting more directly on the south-facing slopes; and abundant rainfall during the hot and windy part of the year—are believed to have broken down and removed the rocks on the north side of the streams more rapidly than those on the south side. The nature of the prevailing strata—interbedded limestone and shale, some interbedded sandstone and shale, and loosely cemented very calcareous sand and gravel beds—is no doubt favorable to the operation of these agents. The greater inwash of material from the north sides than from the south is believed to crowd the streams southward. (*Author's abstract.*)

Discussed by Messrs. SEARS, CAPPS, WHITE, RUBEY, BRADLEY, MATTHES, MISER, THOMPSON, and SCHRADER.

H. D. MISER: *Structure of the Ouachita Mountains of Oklahoma and Arkansas.* The Ouachita Mountains, 200 miles in length and 50 to 60 miles in width, extend westward from central Arkansas into southeastern Oklahoma, about half of the mountain region being included in each State. The rocks, which are mostly shale and sandstone, with some chert and novaculite, range in age from Cambrian to Pottsville (Pennsylvanian) and aggregate in thickness 25,000 feet. All the strata were deformed, presumably in late Pennsylvanian time, by folding and faulting that were produced by compressive movements from the direction of Llanoria in Louisiana and eastern Texas. The crustal shortening in the Ouachita region in Arkansas is about half the original extent of the strata, but in Oklahoma it is apparently greater than half. The shortening in Arkansas is due to close folding and a minor amount of thrust faulting, but the shortening in Oklahoma has been brought about by many long parallel thrust faults, as well as folds. The folds in Arkansas are characteristically isoclinal in large areas, whereas the folds in most of the Oklahoma area are open, though asymmetrical.

Of the several major faults in Oklahoma the two longest, the Choctaw and the Windingstair, reach into Arkansas. They are 125 and 110 miles in length, respectively. The usually accepted idea is that the planes of the

major faults have steep dips—between 30° and 90°. The discovery by the writer in 1927 of a window through an overthrust mass in and near Round Prairie in the Potato Hills, west of Talihina, Okla., indicates the presence of low angle thrust planes in the Ouachita Mountains. The actual horizontal displacement by the fault surrounding the window is at least 3 miles. The fault is interpreted to be the cropping edge of the Windingstair fault plane, which comes to the surface at the south base of Windingstair Mountain, 3 miles north of the hills. The total apparent known extent of the fault plane from Windingstair Mountain into the Potato Hills is 6 miles. The actual extent, of course, exceeds this distance. The plane in this distance is somewhat folded, but the folding of the involved rocks took place for the most part before the thrusting. The discovery of the window leads not only to the obvious conclusion that low angle thrusts exist in the Ouachitas, but points toward the conclusion that the major faults there bound thin slices of the earth's crust.

A greater northward movement of the rocks of the Ouachita region of Oklahoma in comparison with that of the rocks of the same region in Arkansas is suggested by the arcuate forms of the structural trends. In Arkansas most of the trends are west and west-southwest, then along and near the west side of the State they bend to a west-northwesterly direction, and next they swing to the southwest in Oklahoma.

The arcuate trends of the folds and faults in Oklahoma, when considered in connection with the considerable crustal shortening of the rock strata in the Ouachita Mountains, indicate that the rocks of the mountains have been moved in a northerly direction past the east end of the Arbuckle Mountains. The east end of the Arbuckles is, in fact, only about 12 miles from the west end of the Ouachita Mountains.

The proximity of the Ouachitas to the Arbuckles, when considered in connection with their strikingly diverse rock facies, suggests, as has been pointed out by C. L. Dake, that the Ouachita rock section in this part of Oklahoma has been thrust northward a long distance over rocks of the Arbuckle facies. How great the distance is we do not know, but it is perhaps at least 20 miles. (*Author's abstract.*)

Discussed by Mr. ULRICH and Miss JONAS.

CLYDE P. ROSS: *Salient features of the geology of south-central Idaho.* It is now possible to formulate conceptions of the major features of the part of Idaho between 45° 30′ N. latitude and the Snake River Plain. No Archean rocks are known in this region, with the possible exception of some schist near Ivers. There was a great accumulation of marine beds in the Algonkian, at least in Lemhi County, and such beds probably overspread the whole region. These rocks in places are crumpled and overthrust and it appears that much of this deformation is pre-Cambrian. It appears that almost or quite continuously from the late Algonkian or possibly early Paleozoic to the present the region in which the Idaho batholith is now exposed has been above the sea, although in east-central Idaho seas persisted through most of Paleozoic time. Uplift in the part of Idaho where the Idaho batholith outcrops was dominantly a matter of igneous intrusion rather than of folding of strata in a geosyncline.

The Idaho batholith is now exposed over 16,000 square miles and there are numerous exposures of granitic rocks in areas surrounding this large one. Most of the main batholith and its outliers is composed of a characteristic type of quartz monzonite. This rock all came in during one tremendous

period of intrusion. It is at least as old as Cretaceous and present data suggest it may be Jurassic. It is younger than Triassic.

Granite on the Middle Fork of Salmon River was intruded as a sill or irregular laccolith in Miocene or Pliocene time. Sedimentary rock on which this mass rests was converted into injection gneiss through the agency of the Idaho batholith and was again injected by the Tertiary granite, in spite of the thin cover the latter must have had.

The region was eroded nearly or quite to a peneplain and then had stream valleys sharply incised in it before Tertiary volcanism started. After the close of Miocene(?) volcanism the region was again eroded, virtually to a peneplain. Since then the history has been essentially one of intermittent uplift and active erosion. The much discussed "lake beds" along Salmon River are largely water-sorted tuff forming an integral part of the Miocene(?) volcanic series, and their influence on the development of the Salmon River drainage appears to have been of minor importance. (*Author's abstract.*)

Discussed by Messrs. LINDGREN and LOUGHLIN.

W. W. RUBEY, A. A. BAKER, *Secretaries.*

SCIENTIFIC NOTES AND NEWS

The four hundredth meeting of the Chemical Society took the form of a dinner in honor of Dr. F. W. CLARKE, Dr. HARVEY W. WILEY, and Prof. CHARLES E. MUNROE, all of whom were early members of the Chemical Society of Washington which later became the Washington Section of the American Chemical Society. About one hundred and sixty members and guests were present.

The Petrologists' Club met at the Geophysical Laboratory on March 20. D. F. HEWETT discussed *Dolomitization as related to ore deposits*, and C. P. Ross described *Tertiary injection gneisses in Idaho.*

The 9th annual meeting of the American Geophysical Union was held on Thursday and Friday, April 26 and 27, in the National Academy Building. The Sections of Terrestrial Magnetism and Electricity, Seismology, and Geodesy joined on Thursday morning to hear a symposium on *Geophysical methods as applied in the study of geological structure.* The Sections of Meteorology and Oceanography joined on Thursday morning and afternoon to hear a symposium on *Interrelations between the sea and the atmosphere, and the effect of these relations on weather and climate.* This symposium was in three parts: (1) Problems related to solar radiation, (2) Problems related to surface water temperature, and (3) Problems related to atmospheric circulation. The Section of Geodesy met on Friday morning to hear reports on the progress of geodetic work in Mexico, Canada, and the United Stated, and a symposium on *The figure of the earth.* The Section of Volcanology met on Friday morning to hear papers in its field, and the general assembly of the Union was held on Friday afternoon. H. S. WASHINGTON, Chairman, presided over the general meeting, and WILLIAM BOWIE, Geodesy, L. H. ADAMS, Seismology, T. W. VAUGHAN, Oceanography, H. H. KIMBALL, Meteorology, T. A. JAGGAR, JR., Volcanology, presided over the sectional meetings. G. W. LITTLEHALES is Vice-Chairman, and JOHN A. FLEMING General Secretary of the Union.